The Impact of Sudden Changes in Title I Funding on Ohio School Districts

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by

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Abstract

Through the 1965 Elementary and Secondary Education Act (ESEA), the federal government established Title I funding to target resources for districts serving low-income students. The reauthorization of ESEA in 2001 expanded the program's funding significantly, making Title I the largest federal education finance program aimed at improving the achievement of low-income students, yet the impact of this funding on disadvantaged students is still heavily debated. Determining the causal impact of Title I funding on district spending and student learning is difficult because the poverty counts at the center of the funding formula are also correlated with these outcomes. This study exploits the fact that changes in federal student poverty counts due to the 2010 Census, which are unrelated to actual changes in district poverty levels during that same time period, led to significant changes in Title I allocations across districts. I exploit these changes in funding to implement an instrumental variables research design to determine the causal effect of sudden changes in Title I funding on Ohio school districts. This paper finds that sudden changes in Title I allocations increase total, state, and federal revenue initially, but in the long-run a local offsetting response subsequently results in a negative effect for instructional expenditures. The results suggest that Title I has little to no effect on student achievement.

I. Introduction

The passage of the 1965 Elementary and Secondary Education Act (ESEA) represents the most comprehensive federal legislation for education during the 20th century. The act established Title I funding to provide resources for school districts serving high proportions of disadvantaged students. Around the same time, the U.S. Department of Education released "The Coleman Report", a study on the equality of educational opportunity in the United States. The report found that school funding had little impact on student achievement (Coleman, 1966), a controversial finding that has sparked debate about the effectiveness of school finance equalization for decades.

The reauthorization of Title I through No Child Left Behind established four distribution formulas for the program: the Basic Grant, Concentration Grant, Targeted Assistance Grant, and Education Finance Incentive Grant. Each grant has a unique mechanism of distribution. The Basic Grant provides funding to school districts based on the number of students in poverty and accounted for 45% of Title I funding for a total of $6.4 billion in Fiscal Year 2014 (FY 14). The Concentration Grant provides funding to districts with over 15 percent of students in poverty or greater than 6,500 poor children. Approximately 9 percent of Title I funding and $1.4 billion was distributed through the Concentration Grant in FY 14. The Targeted Assistance formula provides increasingly more funding per student as the poverty rate increases across districts. The Education Finance Incentive Grant rewards some states for spending their funds wisely while targeting funds to districts in states that spend inequitably. The Targeted Assistance and Education Finance Incentive Grants each accounted for 23 percent of Title I funding and $3.3

billion in FY 14. With over $14.4 billion in funding provided through Title I in FY 14, the program represents the most significant federal effort to aid low-income students.[1]

The most recent reauthorization of ESEA occurred in December of 2015 with the passage of the Every Student Succeeds Act (ESSA). This new federal education legislation has reignited the discussion on how to most effectively target Title I funds for the benefit of low-income students. In concurrence with ESSA returning greater power and authority over K-12 education to the states, the law also increases states' discretion over how Title I funds are used and broadens the scope of initiatives for which schools can target their resources. Districts also have to show that Title I supplements state and local spending, ensuring that funds received from the federal government do not simply supplant funds from lower levels of government.[2] Title I is unlikely to have its intended effect if state and local governments simply reduce their allocations for education in response to the receipt of Title I funds.

The analysis of the program's impact on student achievement also has larger implications for states seeking to implement school finance reforms that result in greater funding equity for low-income districts. The state of Ohio, along with many others, has faced several lawsuits from plaintiffs arguing that the state's school finance formula does not provide for a "thorough and efficient system of common schools." The state's school funding formula has been ruled unconstitutional four times, and Ohio has pursued a weighted student funding formula in an effort to address the problem.[3] Weighted funding formulas are intended to provide targeted funds to students based on their needs. Districts whose students are considered more expensive to

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[1] "No Child Left Behind – Title I Distribution Formulas". (2014, April 24). New America Foundation. Retrieved from http://febp.newamerica.net/background-analysis/no-child-left-behind-act-title-i-distribution-formulas.
[2] Ujifusa, Andrew. "Funding Flexibility Enhanced Under New K-12 Law". (2016, January 5). *Education Week*. Retrieved from http://www.edweek.org/ew/articles/2016/01/06/funding-flexibility-enhanced-under-new-k-12-law.html.
[3] "Your Guide to the Ups and Downs of School Funding in Ohio". (2013, January 28). NPR StateImpact. Retrieved from http://stateimpact.npr.org/ohio/2013/01/28/your-guide-to-the-ups-and-downs-of-school-funding-in-ohio/.

educate receive more state funding. Analyzing the impact of Title I funding on student achievement may shed light on whether pursuing more equitable finance systems will lead to improved student outcomes.

Although countless studies have been completed in an effort to determine the relationship between school funding and student performance, there is still no consensus among academics on this controversial topic. Many past studies have suffered from endogeneity biases because funding levels are often correlated with other factors, such as poverty, that also have a dramatic effect on student achievement. In this paper, I employ a quasi-experimental research design to address the endogeneity problem and I take advantage of the unique nature of the Title I allocation formulas that generated a sudden shift in funding in the 2012-13 school year (FY 13). Sudden shifts in poverty counts generated by decennial updating of the U.S. Census subsequently drive exogenous changes in Title I funding. I employ a two-stage least squares (2SLS) instrumental variables (IV) research design to estimate the causal effect of exogenous changes in Title I funding on district spending and academic performance in Ohio. My analysis sheds light on whether sudden increases in Title I funds result in future offsetting responses from state and local governments. Furthermore, my analysis on the impact of Title I on student achievement reveals the potential effectiveness of increasing flexibility in how money is spent through mechanisms such as weighted student funding formulas.

The rest of this paper proceeds as follows. In Section II, I review research linking school funding and student outcomes as well as research on intergovernmental grants, including Title I of ESEA. Section III provides an overview of my data and methodology. In Section IV, I discuss my results and Section V offers concluding remarks.

II. Literature Review

A. School Funding and Student Outcomes

The impact of funding on student outcomes has been a heavily debated topic in education policy for decades. Hundreds of studies have utilized education production functions to estimate the relationship between school inputs, such as teacher salary and teacher-pupil ratios, and school outputs in the form of student test scores or graduation rates. Through a meta-analysis of nearly 400 studies, Hanushek (1996) determined that there was not a strong or consistent relationship between student performance and school resources. This finding may seem counterintuitive, but Hoxby (1996) argues that teachers' unions lobby for increased school inputs while negatively impacting student performance. Although Hoxby's study may provide a plausible explanation of perverse incentives in the education system, the relationship between resources and achievement is far from a settled question. Another comprehensive meta-analysis (Greenwald, Hedges, & Laine, 1996) found that a broad range of resources had a substantial positive correlation with academic outcomes. The authors claim that moderate increases in spending are likely to lead to significant increases in academic achievement. However, because of limitations of the research design in studies using education production functions, the causal relationship between expenditures and student outcomes has largely eluded researchers until recently.

Many studies linking resources and achievement suffer from endogeneity biases and fail to estimate the true causal effect of funding on academic outcomes, but recent studies have employed more advanced research methodologies in an effort to estimate the causal effect of school funding on student outcomes. Researchers have utilized quasi-experimental research designs including IV techniques, regression discontinuity, and many others in an effort to answer

this question. The results of several more recent studies, which use newer methodologies and evaluate the impact of school finance reforms seeking to create more equitable and adequate levels of funding, appear to challenge Hanushek's findings (1996).

Jackson, Johnson, and Persico's recent study (2014) estimates the effect of changes in school funding driven by court-mandated school finance reforms on fiscal, academic, and economic outcomes. Utilizing a comprehensive database on school finance reforms from 1967-2010, the authors assess the impact of various reforms on low and high-income school districts. The reforms have played a key role in equalizing spending among these types of districts by increasing spending in low-income districts. The authors used an IV-2SLS research design based on the timing and intensity of court-mandated school finance reforms. They found that the effects of a 20 percent increase in per-pupil spending each year for all 12 years of public school for children from poor families are large enough to eliminate most of the gaps in the adult outcomes between those raised in poor families and those raised in higher income families.

Card and Payne (2002) also found promising results for school finance equalization advocates. The authors found that court-mandated school finance reforms led to increases in funding available for lower-income districts, which subsequently resulted in a $0.30-0.65 increase in overall spending for each additional dollar in state aid that was received. Equalization of spending also led to a modest equalizing effect on test score outcomes between students whose parents come from higher and lesser educated backgrounds. Another study by Guryan (2001) assessed the impact of a major school finance equalization scheme on student achievement in Massachusetts. Exploiting discontinuities in the staid aid formulas of the Massachusetts Education Reform Act of 1993, Guryan found that increased funding had positive impacts on some student test score outcomes.

Two studies found positive attainment and test outcomes as a result of a school finance reform in Michigan. Hyman (2013) employed an IV-2SLS research design to estimate the effect of Michigan's Proposal A on long-run educational attainment. The author found that increasing spending led to higher rates of college entry and postsecondary degree completion, but benefits were primarily concentrated among non-poor students. Roy (2003) performed a similar analysis on Proposal A in Michigan to assess the effect of legislative-led school finance reform on short-run outcomes – the author found that the reform was successful in reducing spending disparities between school districts and led to significant gains in scores on state tests for the lowest spending districts that were the primary beneficiaries of the funding reform.

In the most recent comprehensive study on school finance reforms, Lafortune, Rothstein, and Schanzenbach (2016) studied the impact of court-mandated and legislative reforms from 1990-2013. Finance reform events led to sustained increases in spending in low-income districts with large positive impacts on student achievement – a $1,000 per pupil increase in annual spending in a low-income school district led to a 0.18 standard deviation increase in test scores 10 years later. However, the authors' findings come with an important caveat: although inequities between high and low-income districts were reduced, there was no effect on within-district academic inequities between high and low-income students. These studies are mostly uniform in their finding that school finance reforms led to spending equalization that has positive impacts on student achievement. The fact that these studies using rigorous methodologies have consistently challenged Hanushek's findings (1996) re-opens the question about the relationship between school resources and student outcomes. Because my study focuses on the impact of federal Title I funding, a review of the literature on intergovernmental grants and research on the impact of Title I also provides valuable context for my findings.

B. Intergovernmental Grants and Title I Funding

When states and localities receive funding from a higher level of government, there is potential that they will supplant the funds, reducing their own spending in response to funding received from an external source. However, a large body of evidence seems to confirm the "flypaper effect", which suggests that funding from a higher level of government increases spending on a targeted program more than theory would predict (Hines & Thaler, 1995). To the contrary, more recent studies have begun to challenge the consensus view on the flypaper effect. Knight (2002) found that federal highway grants crowd out state-spending, while Gordon (2004) found that exogenous changes in Title I funding caused educational expenditures to increase in the short run, but level off over time as a local offsetting response reduced local spending. These results suggest that the impact of intergovernmental grants on spending at lower levels of government warrants further research.

A number of studies assess the effect of Title I funding on state and local spending as well as student performance. In "Prospects: The Congressionally Mandated Study of Educational Growth and Opportunity", Puma et al. (1993) found that Title I funds did not close the gap in student achievement between disadvantaged students and their more advantaged peers. However, there are two main methodological issues with these findings. First, it is entirely possible that disadvantaged students would have been even worse off and the achievement gap would have widened in the absence of the supplementary funds. Borman and D'Agostino's meta-analysis (1996) of 17 studies on the impact of Title I found an overall positive effect of Title I on student achievement and that the program improved over time through better enforcement and program improvement standards. Although it is unlikely that Title I can eliminate the performance gap between high and low-income students, these findings suggest that disadvantaged students would

have fallen even further behind in the absence of Title I. Second, the study does not account for a potential offsetting effect from state and local governments in response to the receipt of Title I funds that may result in no increase in net services for disadvantaged students. Studies on the impact of Title I on net revenues and spending have come to differing conclusions. Feldstein (1978) found that for each dollar of Title I funds received, local school districts increase their spending by 72 cents, suggesting that a substantial portion of the funds are used to increase spending. To the contrary, Gordon (2004) found that while Title I initially boosts revenue and spending, these results are no longer significant and their effect is reduced three years later because of a local offsetting response. These results indicate that the crucial question about whether Title I funds increase net revenues and services for disadvantaged students is still open for debate.

A few more recent studies have employed a regression discontinuity design (RDD) to assess the impact of exogenous changes in Title I funds. The studies exploit an arbitrary cutoff in the Title I distribution formula that allows schools above a certain level of poverty to receive Title I funding, while schools below the threshold receive no funding. In a study on Title I in New York City, overall spending increased while test scores actually declined for the treatment group that received Title I funding (Weinstein, Stiefel, Scwartz, & Chalico, 2009). Van der Klaauw (2008) found that although Title I funding increased for the treatment group, it did not lead to a net increase in overall spending and had an adverse effect on student achievement. Although one study did find some evidence of crowding out of state and local funds, total net revenues increased and had no effect on student achievement (Matsudaira, Hosek, & Walsh, 2012). In sum, two of the three studies found evidence of a net increase in spending, two of the

three studies found evidence of crowding out of spending at lower levels of government, and all of the studies found either a negative effect or no effect on student achievement.

Most relevant to my analysis, Gordon (2004) used an IV-2SLS research design to estimate the impact of Title I funding on district revenue and spending. Gordon exploits the decennial updating of poverty counts in the 1990 U.S. Census, which generated an exogenous change in Title I funds when the new poverty counts were first implemented in the 1993 allocation formula. These exogenous changes in funds instrument for the actual changes in Title I funding to estimate the causal effect on revenues and spending. Gordon initially found large, positive, statistically significant increases in total revenue and instructional spending in the first year, but those effects were reduced substantially and became insignificant as a result of a large, negative, statistically significant offsetting effect in local revenue three years after the new poverty counts were used. My analysis utilizes a similar methodology to Gordon's, but I use updated poverty counts from the 2010 U.S. Census.

III. Empirical Approach

Numerous studies have come to differing conclusions about the impact of Title I, so my analysis seeks to address two primary issues: the impact of sudden changes in Title I funding on state and local spending as well as the subsequent impact of those shifts in funds on student achievement. Fortunately, the discontinuous nature of Title I funding that is the driven by changes in poverty counts from the 2010 U.S. Census presents an opportunity to use an IV-2SLS research design to more effectively determine a causal relationship in addressing these two issues in question. My analysis yields findings that contribute to the public economics literature on fiscal federalism and the education economics literature on education production functions. Studies have consistently found evidence of the flypaper effect: funds from a higher level of

government tend to "stick" to the purpose for which they are allocated (Hines & Thaler, 1995). Gordon's findings (2004) challenge the findings in this literature, and my analysis also reveals the deeper complexities about how state and local governments alter their funding patterns for education over time in response to exogenous influxes of funds from the federal government.

I also obtained estimates of the impact of those fiscal effects on student achievement. Assessing the effect of these shifts in funds on student performance furthers Gordon's research, which only focuses on revenues and expenditures. An advantage of focusing on Ohio is the availability of value-added estimates of school districts' impacts on student achievement. These estimates capture student learning growth and effectively control for student characteristics, providing a superior measure of district performance than that used in prior studies. Chetty, Friedman, and Rockoff (2013) found that value-added scores provide unbiased estimates of teachers' causal impacts on student performance, so the use of this data is a significant improvement on previous literature on the relationship between school funding and student outcomes that does not utilize value-added student achievement data.

A. Data

This study uses district-level panel data from 2010-2015 for over 600 Ohio school districts. The Ohio Department of Education provides publicly available data on over 50 variables in District Profile Reports. The data include basic revenue and expenditure measurements; important measures of student characteristics, including percentage of economically disadvantaged students; and measures of real resource variables such as average teacher salary and teacher experience. Data on value-added test scores for each Ohio school district were also obtained from the Ohio Department of Education. Value-added measures student learning gains for each district in a given year by accounting for multiple years of prior

student test scores, thereby accounting for differences in characteristics between students (Ohio Department of Education, 2014).

Title I poverty and allocation data for 2010-2015 were obtained from the National Center for Education Statistics (NCES). Title I funding is dependent on a given state or district's eligibility count, defined as the number of students in poverty. The eligibility count is the number of children, aged 5-17, who live in:

1. Families with income at or below the poverty level (Department of Commerce)
2. Families with incomes above the poverty level, but who receive Temporary Assistance for Needy Families (TANF)
3. Institutions for neglected or delinquent children
4. Foster homes in which the parents receive government payments for the child's support

NCES records the poverty counts, enrollment, and allocations for Ohio school districts for the four Title I grant formulas: Basic, Concentration, Targeted, and Education Finance Incentive Grants (Sonnenberg & Provasnik, 2007).

B. Identification Strategy

A standard ordinary least squares (OLS) model would assess the impact of changes in Title I funding on changes in a particular outcome of interest to assess whether Title I exhibits flypaper properties. Consider this basic OLS specification:

(1) $\Delta Y_{dt} = \beta_0 + \beta_1 \Delta ActualTitleI_{dt} + \mu_{dt}$

where d indexes districts and t indexes time. ΔY_{dt} is the one year change in an outcome, $\Delta ActualTitleI_{dt}$ is the actual one year change in Title I funds, and μ_{dt} is the random error term. However, this OLS specification suffers from omitted variable bias and violates one of the basic assumptions of the model – that the independent variable is uncorrelated with the error term. Poverty counts are the primary factor determining Title I allocations, and district poverty level also has a substantial effect on several outcome variables, particularly student achievement. Thus

a research design that exploits exogenous changes in Title I funding is required to obtain casual estimates for the outcomes of interest.

My identification strategy essentially replicates Gordon's method (2004), but I use updated poverty data from the 2010 Census to assess the causal effect of sudden changes in Title I funds on revenues, spending, and academic outcomes for Ohio school districts in the 2012-2014 school years. Although NCES now approximates district poverty counts from year to year instead of using poverty counts dating all the way back to the 2000 Census, there was still a significant shift in poverty and funding for many districts when the new data were first used in the Title I allocation formulas in 2012-13 (or FY 13). With the use of updated data, reported poverty counts jumped discretely, while actual poverty rates and state and local revenues that depend on poverty changed continuously. Most other revenue and spending is related to district property wealth, which varies continuously and is unlikely to have a discrete jump at the same time as the sudden change in Title I funding driven by Census-updating. Sudden shifts in poverty counts generated by decennial updating of the U.S. Census in 2010 subsequently drive substantial exogenous changes in Title I funding.

Table 1 and Figure 1 show the distribution of the change in Title I funding per pupil for 2011-12 (before Census-updating) and 2012-13 (after Census-updating). Although the change at the mean is less than $20 per pupil, the changes at the tails of the distribution are much larger and impactful. The changes in funding were greater in 2012-13 than in 2011-12, and the disparity between the 2012-13 change and the 2011-12 change is greatest at the tails of the distribution. These changes in funds provide the variation needed for the IV method I utilize for my analysis.

C. Creating the Instrument

To isolate the Census-driven changes in Title I funding, I used a two-step simulation process:

1. Calculated what the Title I allocation for FY 13 would have been had the Census not been updated
2. Subtracted the FY 13 predicted allocation for Title I from the FY 13 actual allocation for Title I to obtain the exogenous change in Title I funds for that year

For this simulation process, I ultimately decided to use the allocations for just the Basic and Concentration Grants. The allocation formulas for these grants are much more straightforward and easier to replicate since funding is awarded based primarily on the eligibility count (denoted $Poor_{Year}$) and state per pupil expenditure (denoted $SPPE_{Year}$), while the formulas for the Targeted and EFIG Grants are much more complicated and provide increasingly more funding per student as the poverty rate increases across districts. Also, the Basic and Concentration Grants still account for a majority of Title I funding, so using just these two grants provides substantial variation in funding to generate the IV.

$Poor_{Year}$ and $SPPE_{Year}$ are the main factors that determine simulated allocations for the Basic and Concentration Grants. In the first step of the simulation, I used the formula that NCES employs to calculate $SPPE_{Year}$:

(2) (Eligibility Count)*(Adjusted SPPE) = Allocation Amount

For example, I divided $Allocation_{2013}$ by $Poor_{2013}$ to obtain $SPPE_{2013}$. I then used $SPPE_{2013}$ and multiplied it by $Poor_{2012}$ to obtain the FY 13 predicted allocation – this calculation uses FY 12 poverty counts and FY 13 SPPE to determine what the FY 13 allocation would have been had the Census poverty counts not been updated, holding SPPE constant. I then used simple differencing methods, as described in step two, to obtain the exogenous change in Title I funds.

This simulation method is detailed with the following equations. The simulated variable is the Census-determined change in Title I per pupil:

(3) $\frac{TI2013(Poor2013,SPPE2013)}{Enrollment2013} - \frac{TIsim(Poor2012,SPPE2013)}{Enrollment2012}$

The change in (3) serves as an IV for the actual change in Title I funds for all four grants:

(4) $\frac{TI2013(Poor2013,SPPE2013)}{Enrollment2013} - \frac{TI2012(Poor2012,SPPE2012)}{Enrollment2012}$

These variables are used to obtain estimates for outcomes that are also based on a change from FY 12 to FY 13. For the two-year change, the simulated variable for the Census-determined change in Title I:

(5) $\frac{TI2014(Poor2014,SPPE2014)}{Enrollment2014} - \frac{TIsim(Poor2012,SPPE2014)}{Enrollment2012}$

The change in (5) serves as an IV for the actual change in Title I funds for all four grants:

(6) $\frac{TI2014(Poor2014,SPPE2014)}{Enrollment2014} - \frac{TI2012(Poor2012,SPPE2012)}{Enrollment2012}$

These variables are used to obtain estimates for outcomes that are also based on a change from FY 12 to FY 14. However, attempting to calculate the exogenous change in funding from 2012-14 introduces measurement error in the IV because the exogenous change occurs just when new Census poverty counts were implemented in FY 13. In an effort to deal with this methodological issue, I also use the FY 13 exogenous change in Title I funds to instrument for the actual change in Title I funds. I estimate models using the IV in both equation 5 (see Table 3, column 4) and equation 7 (see Table 3, column 2) for comparison, but the discussion of my results focuses on the estimates using the IV in equation 7 since it reduces measurement error:

(7) $\frac{TI2013(Poor2013,SPPE2013)}{Enrollment2013} - \frac{TIsim(Poor2012,SPPE2013)}{Enrollment2012}$

The change in (7) serves as an IV for the actual change in Title I for all four grants:

(8) $\frac{TI2014(Poor2014,SPPE2014)}{Enrollment2014} - \frac{TI2012(Poor2012,SPPE2012)}{Enrollment2012}$

These variables are used to obtain estimates for outcomes that are also based on a change from FY 12 to FY 14.

It is important to note that there are two aspects of the Basic and Concentration Grant distribution formulas that may introduce some measurement error in the simulation method I employ. The hold-harmless clause ensures that districts do not incur exceedingly large losses of funding compared to the previous year as a result of large drops in eligibility counts. Although the hold-harmless may limit drastic losses in funding for districts with declining eligibility counts with the new Census data, the IV still generates adequate variation for my analysis. The Small State Minimum also ensures that no state should receive less than a minimum threshold of funding (Sonnenberg & Provasnik, 2007), but since my analysis is limited to the state of Ohio, it seems unlikely that this factor would drive significant differences between districts within the state.

D. Instrumental Variables – Two-Stage Least Squares Research Design

The analysis uses the simulated funding changes calculated above as IVs in 2SLS statistical models. I use the calculated exogenous changes in Title I funds to predict actual changes in Title I funds in the following first-stage model:

(9) $\Delta ActualTitleI_{dt} = \beta_0 + \beta_1 \Delta ExogTitleI_{dt} + \mu_{dt}$

$\Delta ActualTitleI_{dt}$ is the actual change in Title I funds, $\Delta ExogTitleI_{dt}$ is the calculated change in Title I funds due to changes in the 2010 Census, and μ_{dt} is the random error term. I then use the change in funding predicted by the first-stage result in my second-stage model:

(10) $\Delta Y_{dt} = \beta_0 + \beta_1 \Delta ActualTitleI_{dt} + \beta_2 lag\Delta State\ Revenue_{dt-1} + \beta_3 lag\Delta State\ Revenue_{dt-2} + \beta_4 lag\Delta LocalRevenue_{dt-1} + \beta_5 lag\Delta Local\ Revenue_{dt-2} + \mu_{dt}$

where ΔY_{dt} is the change in outcome, $\Delta ActualTitleI_{dt}$ is the change in funding predicted by the first-stage result, and the model includes controls for lagged one year changes in revenue per pupil in FY 11 and FY 12, the years immediately preceding the use of new poverty counts.[4] All outcome and explanatory variables are based on per pupil measurements. The analysis estimates the above models across a number of outcomes, including the impact of Title I funds on revenue variables including total, state, local, and federal revenue; expenditure variables, including total expenditure and instructional expenditure; and student achievement variables, including value-added gain and the performance index.[5] I estimate these models for both the one year change from FY 12 to FY 13 and the two year change from FY 12 to FY 14.

IV. Results

I consider both short-term (one year change) and long-term (two year change) responses to exogenous changes in Title I funds to test the flypaper effect over time. First, I discuss quality of the calculated instrument as a predictor for Title I based on first-stage results. Second, I examine both the short and long-run impacts of Title I from the second stage results, and my discussion focuses primarily on columns 1 and 2 in Table 3 since those are the columns that use the IV for FY 13. Third, I compare my IV-2SLS estimates to OLS estimates. Finally, I discuss results from specification tests on pre-period outcomes to justify the use of my IV as an exogenous source of Title I funding.

[4] Gordon (2004) also controls for enrollment changes in her second-stage specification. I provide results of a model that controls for enrollment changes in Table 6, which shows that the sign and significance of the estimates correspond closely to the results provided in Table 3 based on the specification in equation (10).
[5] Descriptive statistics for variables from both the Ohio Department of Education and NCES are provided in Tables 7 and 8.

A. First-Stage Results

Table 2 shows my first-stage results, and it is clear that the simulated change in Title I funds is a strong predictor for the actual change in Title I funds for both the one and two year changes. In column 1, the actual change in Title I from FY 12 to FY 13 serves as the dependent variable, while the simulated change in Title I for the same time period serves as the independent variable for the regression. The coefficient for the calculated change is 1.19, so a $1 increase in simulated exogenous changes in Title I leads to a $1.19 increase in the actual changes in Title I funds. This result is statistically significant at the 1% level. The F-statistic is also large with a value of 814.29, and the R-squared value of 0.57 shows that the simulated change explains a large proportion of the actual change. In column 2, the actual change in Title I from FY 12 to FY 14 serves as the dependent variable, while the simulated change in Title I for the same time period serves as the independent variable for the regression. The coefficient for the simulated change is 1.39, which means a $1 increase in exogenous changes in Title I funds translates to a $1.39 increase in the actual changes in Title I funds. This estimate is also statistically significant at the 1% level. Similar values for the F-statistic and R-squared are obtained for this result as well. With a coefficient greater than one that is statistically significant and a large R-squared value in both cases, the IV has strong predictive power.

B. Second-Stage Results: Short-Term Responses to Title I

The second-stage results indicate that initially Title I funds stick to the purpose for which they are intended. I emphasize focusing on the direction and significance of the coefficients rather than strict interpretation of the results because of inherent measurement error in the analysis. In column 1 of Table 3, I report estimates of the impact of Census-induced changes in Title I on the outcomes of interest. A $1 dollar increase in Title I translates into more than a $2

increase in total revenue, a $1.75 increase in state revenue, and a $0.85 increase in federal revenue. Considering the revenue results for column 1 of Table 3, when the coefficients for state, local, and federal revenue are added, they sum perfectly to the coefficient for total revenue, which suggests that the results are sensible. The large and positive result for state revenue warrants further attention. The state of Ohio has made numerous changes to the state funding formula in the last five years, so the result may be a reflection of the state pushing greater funding to low property wealth districts that are the same beneficiaries of sudden increases in Title I funding. It may be the case that the state incorporates Census data into its allocation formula, but this information is not well documented.

I also find smaller, positive, statistically insignificant effects for local revenue, total expenditures, and instructional expenditures. Although the results for expenditures are not significant, the direction of the coefficients indicates that increases in total and state revenue translate to increases in instructional expenditures. This creates the opportunity to provide supplemental support to low-income students through classroom aides, tutoring, and a variety of other instructional interventions schools can take with increased discretional spending. However, there is also a very small, negative, statistically significant effect for the performance index, but the effect is so small that it is unlikely to be large enough in magnitude to have any substantial impact on student learning. Essentially Census-driven changes in Title I have little to no impact on student performance in the short-term. This finding is unsurprising when the magnitude of and the time period for the change in Title I funding is considered. The descriptive statistics in Table 8 reveal that for the exogenous and actual change in Title I funds, the average change is less than $20 per pupil, and even for the districts that gain or lose the most funding, the change in funds is rarely more than a few hundred dollars per pupil. This change in funding pales in

comparison to the magnitude of the spending change considered in other studies. For example, Jackson, Johnson, and Persico (2014) assess the effect of a 20 percent increase in per-pupil spending and Lafortunte, Rothstein, and Schanzenbach (2016) evaluate the impact of a $1,000 increase in per-pupil spending. Furthermore, both studies consider the effect of these spending increases over the course of a decade or more. The Title I funding change driven by Census-updating is unlikely to be economically significant enough to have a dramatic impact on schooling outcomes, especially over such a short period of time.

C. Second-Stage Results: Long-Term Responses to Title I

Although I initially find results indicative of flypaper properties, the estimates in column 2 of Table 3 reveal that the positive effects for total and state revenue are reduced for the two year change and there is a negative local offsetting effect. The Census-induced change in Title I results in a $0.55 increase in total revenue, $1.15 increase in state revenue, $0.53 decrease in local revenue, and a $0.59 increase in federal revenue. Only the results for state and federal revenue are significant. For expenditures, the positive effect of total expenditures is reduced, while the estimate for instructional expenditures becomes negative. These estimates indicate that offsetting effects in revenue may translate to offsetting effects in expenditures and result in reduced instructional spending. The size of the negative effect for the performance index nearly doubles in size and is statistically different from zero, which may indicate that losses in revenues and instructional expenditures translate to slight negative effects on student achievement. Overall, the short-run impact of sudden changes in Title I funds results in revenue and expenditure increases, confirming the flypaper effect; however, over time, the positive effects for revenue are reduced and there is a negative local offsetting response that has adverse impacts on instructional spending and student achievement.

I also provide alternative sets of estimates for the two year change based on different specifications. In column 4 of Table 3, the FY 14 IV is used for the actual change in Title I from FY 12 to FY 14 to assess the impact on the change in outcomes from FY 12 to FY 14. In column 3, both the FY 13 and FY 14 IVs are used to instrument for the actual change in Title I from FY 12 to FY 14 to assess the impact on the change in outcomes from FY 12 to FY 14. I focused the discussion of my results on the estimates in column 2 because that IV most closely captures the exogenous shock in funding from FY 12 to FY 13, but I also wanted to provide estimates based on the simulated exogenous change in funding from FY 12 to FY 14 since the IV is used for actual changes in funding from FY 12 to FY 14. However, the result for federal revenue in column 4 calls into question the use of the FY 14 IV. A $1 increase in Title I translates to just a $0.19 increase in federal revenue when Title I accounts for a majority of federal funding, and the result is not statistically significant. I also find a large, negative effect for local revenue as well as a positive effect for state revenue, and both of these results are significant. When the IVs for the exogenous change from FY 12 to FY 13 and FY 12 to FY 14 are both used (in column 3), I find that the estimates are approximately midway between the estimates for specifications in which just one IV is used (in columns 2 and 4). The results suggest that simulating the exogenous change in funding from FY 12 to FY 14 introduces measurement error in the IV since no exogenous change in funding occurs from FY 13 to FY 14.

D. OLS Results

Table 4 shows the OLS estimates. The direction of the effect is the same as the IV estimates except for the one year change for local revenue and the two year change for total expenditure (neither of these variables is significant for both the IV and OLS estimates). The size of the effects is also similar in most cases. Generally the OLS estimates indicate that sudden

changes in Title I initially increase revenues and spending, but lead to a negative offsetting effect in local revenue that translates to a negative effect for instructional expenditures.

E. Specification Tests

To justify the use of my IV, I employ specification tests that provide evidence that the coefficients from my IV-2SLS analysis can be interpreted as causal estimates. Through my specification tests, I consider the impact of the changes in Title I per pupil from FY 12 to FY 13 on changes in the outcome variables from FY 11 to FY 12 (before Census-updating). Table 5 shows that the exogenous change in Title I from FY 12 to FY 13 has no effect on pre-period changes in revenue, expenditures, and student achievement. The standard errors indicate that not a single estimate is statistically different from zero. The specification tests suggest that the use of the IV as an exogenous source of funding is legitimate.

V. Conclusions

This paper finds evidence of typical flypaper properties initially, with both total and state revenue increasing with changes in Title I, but in the long-run a negative local offsetting response results in a reduction in the positive effect for total revenue and a negative effect for instructional expenditures. I also find a slight, statistically significant, negative effect for the performance index for both the short and long-term changes in Title I, but the effect is so small it is unlikely to have a substantial academic effect. The effect for value-added gain is insignificantly different from zero.

These results advance the pubic economics literature on the flypaper effect. By considering both one and two year changes in funding, I am able to assess the "stickiness" of Title I over time. In the first year, there appears to be significant crowd-in with Title I, with a dollar increase in Title I translating into more than a dollar increase in both total and state

revenue. However, the two year change in funding has a negative impact on local revenue and the impact on total revenue becomes insignificantly different from zero, providing some evidence of crowd-out. It may seem surprising that there is evidence of an offsetting response in just two years, but Ohio's system of school funding may establish circumstances for an offsetting response to be realistic in a fairly short time period. Voters routinely face levies on the ballot every year, [6] and a local school board could act on information about a sudden increase or decrease in Title I funds to determine if a levy should be proposed.

Curiously, there is still significant crowd-in for state revenue for the two year change. A potential explanation for this finding is that this may reflect greater effort by the state of Ohio to push higher levels of funding to the same districts that gained from Census-induced changes in Title I funds. For example, the state passed new budgets in 2011 and 2013 that revised the state school funding formula. The 2011 budget repealed the Evidence-Based Model and put Bridge Funding in place. Bridge Funding provided allocations to districts based on their per pupil funding for FY 11, adjusted and indexed by relative tax valuation per pupil, which is a reflection of district property wealth. The state was also required to provide supplemental funding to make up for the loss of federal stimulus funding (Parker & Cummiskey, 2011). The state passed a new budget again in 2013, creating a new weighted student funding formula that provides funding based on a state share index related to district property wealth and median income (Parker & Cummiskey, 2013). The positive effects for state revenue may be a reflection of legislative changes that provide increasingly more funding to low property wealth districts, the same

[6] "Administrators Blame Ohio's School Funding Model For Levies". (2011, November 4). NPR StateImpact. Retreived from http://www.ideastream.org/stateimpact/2011/11/04/administrators-blame-ohios-school-funding-model-for-levies.

districts likely experiencing large gains in Title I funds. The responses of both state and local governments over time provide valuable information about the flypaper effect.

The findings of this paper also have broader implications for the education economics literature on education production functions by exploring the impact of Title I on student performance. Although the results for expenditures were initially positive, the two year change in funds induced a reduction in instructional expenditures. Title I had either a slight negative or no effect on student achievement. In the long-run, the results suggest that it seems unlikely that Title I ensures a net service increase for low-income students, which defeats the intended purpose of the program of providing greater resources for districts with higher proportions of disadvantaged students. The failure to increase net services may partially explain why Title I does not have a substantial or significant impact on student achievement. Still the achievement results reveal that districts that received exogenous changes in Title I funds were not able to improve student performance, which raises the question of whether the most effective interventions are being used with the flexible use of Title I funds.

Although my analysis sheds some light on the effect of Title I on student outcomes, there is still considerable room for more research. Further research should explore how exogenous changes in Title I funds affect these outcomes over a longer period of time because it may take several years before drastic changes in funding have a substantial impact on student outcomes. Performing analysis that assesses how Title I funds are allocated to schools within a district as well as whether the funds are used for interventions that directly target the neediest students within schools could also reveal important findings. Providing policymakers with information on these issues will be imperative to ensuring efficient and effective use of federal education dollars targeted to help low-income students.

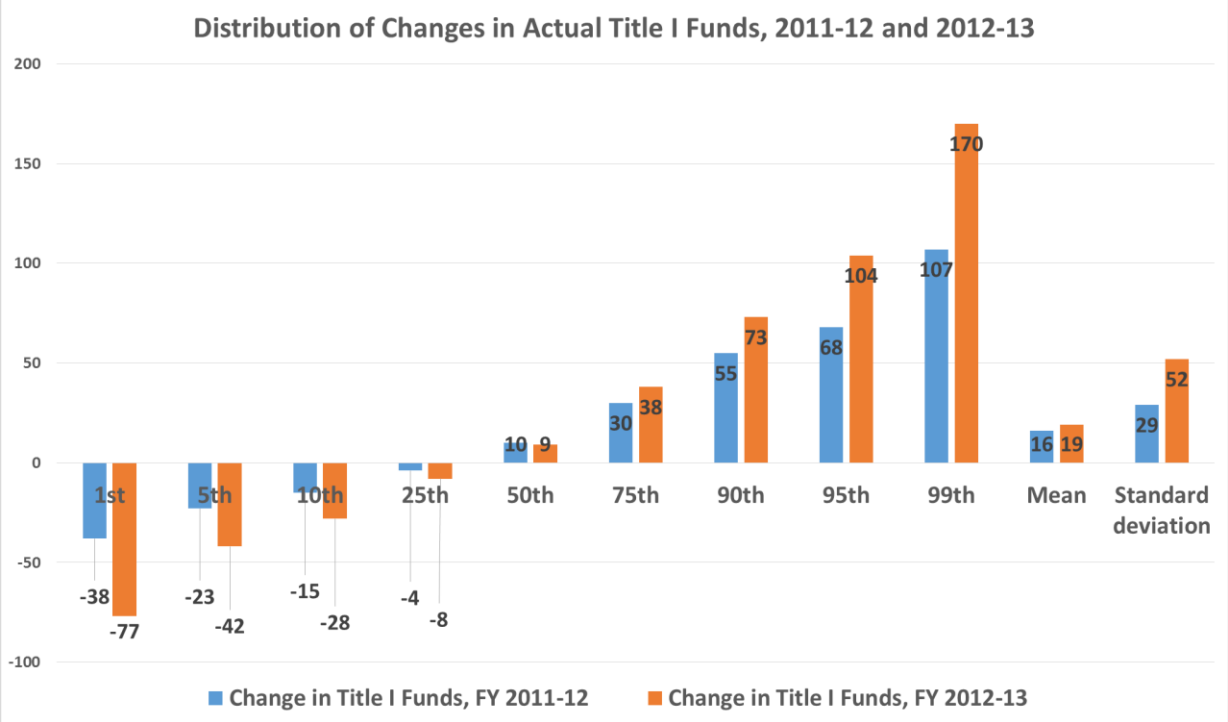
Tables and Figures

Table 1
Distribution of Changes in Actual Title I Funds Per Pupil, 2011-12 and 2012-2013

	Change in Title I Funds, FY 2011-12	Change in Title I Funds, FY 2012-13
1st	-38	-77
5th	-23	-42
10th	-15	-28
25th	-4	-8
50th	10	9
75th	30	38
90th	55	73
95th	68	104
99th	107	170
Mean	16	19
Standard deviation	29	52
N	609	609

Note: the changes in Title I funds are in dollar amounts and the distribution ranges from the 1st to the 99th percentile.
Source: National Center for Education Statistics Title I Allocations, 2015.

Figure 1



Note: the changes in Title I funds are in dollar amounts and the distribution ranges from the 1st to the 99th percentile.
Source: National Center for Education Statistics Title I Allocations, 2015.

Table 2

First-stage results: correlations between simulated exogenous changes in Title I and actual changes in Title I revenue per pupil

Outcomes	(1) 1 year change (2012-13)	(2) 2 year change (2012-2014)
Actual Title I	1.190***	1.393***
	(0.0417)	(0.0495)
F	814.29	793.73
R-Squared	0.5729	0.5667
N	609	609

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ and standards errors are in parentheses.

All regressions control for district-level changes in state and local revenue per pupil from 2010-2012, but are not sensitive to the exclusion of these controls.

Table 3

IV estimates of effects of changes in Title I funds per pupil on changes in revenue and expenditure per pupil, value-added gain, and the performance index

Outcomes	(1) 1 year change (2012-13)	(2) 2 year change (2012-2014)	(3) 2 year change (2012-2014)	(4) 2 year change (2012-2014)
Total Revenue	2.761***	1.210	0.595	0.0365
	(0.827)	(1.060)	(0.995)	(1.049)
State Revenue	1.753***	1.148*	1.185**	1.218*
	(0.498)	(0.632)	(0.594)	(0.625)
Local Revenue	0.159	-0.525	-0.968	-1.370**
	(0.572)	(0.660)	(0.620)	(0.654)
Federal Revenue	0.850***	0.587***	0.378**	0.189
	(0.182)	(0.185)	(0.173)	(0.181)
Total Expenditure	0.420	0.273	-0.121	-0.479
	(0.745)	(1.011)	(0.950)	(1.001)
Instructional Expenditure	0.333	-0.0802	-0.415	-0.719
	(0.465)	(0.578)	(0.543)	(0.572)
Gain	0.00107	0.0000243	0.000772	0.00145
	(0.00181)	(0.00169)	(0.00159)	(0.00167)
Performance Index	-0.00246*	-0.00428**	-0.00402**	-0.00378**
	(0.00140)	(0.00168)	(0.00158)	(0.00166)
N	608	608	608	608

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ and standard errors are in parentheses.

Each cell in the table represents its own regression. New poverty data from the 2010 Census was first used in 2012-13 (or FY 13). Calculated changes in Title I instrument for actual changes in Title I. All regressions control for district-level changes in state and local revenue per pupil from 2010-2012. In column (1), the FY 13 IV is used for the actual change in Title I from FY 12 to FY 13 to assess the impact on the change in outcomes for FY 13. In column (2), the FY 13 IV is used for the actual change in Title I from FY 12 to FY 14 to assess the impact on the change in outcomes from FY 12 to FY 14. In column (4), the FY 14 IV is used for the actual change in Title I from FY 12 to FY 14 to assess the impact on the change in outcomes from FY 12 to FY 14. In column (3), both the FY 13 and FY 14 IVs are used for the actual change in Title I from FY 12 to FY 14 to assess the impact on the change in outcomes from FY 12 to FY 14. OLS results for these specifications are in Table 4.

Table 4
OLS estimates of effects of changes in Title I per pupil on changes in revenue and expenditure per pupil, value-added gain, and the performance index

Outcomes	(1) 1-year change, 2012-2013	(2) 2-year change, 2012-2014
Total Revenue	2.322***	0.449
	(0.634)	(0.792)
State Revenue	2.147***	1.012**
	(0.382)	(0.472)
Local Revenue	-0.361	-0.696
	(0.439)	(0.493)
Federal Revenue	0.535***	0.133
	(0.139)	(0.137)
Total Expenditure	0.524	-0.00482
	(0.572)	(0.756)
Instructional Expenditure	0.342	-0.446
	(0.357)	(0.432)
Gain	0.00218	0.00181
	(0.00139)	(0.00126)
Performance Index	-0.00124	-0.00141
	(0.00108)	(0.00125)
N	608	608

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ and standards errors are in parentheses.
Each cell in the table represents its own regression. New poverty data from the 2010 Census was first used in 2012-2013 (or FY 2013). All regressions control for district-level changes in state and local revenue per pupil from 2010-2012.

Table 5
IV estimates of effects of changes in Title I per pupil (2012-13) on earlier (2011-12) changes in revenue and expenditure per pupil, value-added gain, and the performance index

Outcomes	(1) Pre-period change
Total Revenue	0.148
	(0.601)
State Revenue	-0.0919
	(0.362)
Local Revenue	0.223
	(0.389)
Federal Revenue	0.0172
	(0.189)
Total Expenditure	0.215
	(0.742)
Instructional Expenditure	-0.0408
	(0.434)
Gain	0.000618
	(0.00170)
Performance Index	0.00147
	(0.00151)
N	609

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ and standard errors are in parentheses.
Each cell in the table represents its own regression. New poverty data from the 2010 Census was first used in 2012-13 (or FY 13). Calculated changes in Title I for FY 13 instrument for actual changes in Title I for FY 13, and IV-2SLS is used to estimate the impact on the change in outcomes from FY 11 to FY 12, referred to as the pre-period change.

Table 6

IV estimates of effects of change in Title I funds per pupil on changes in revenue and expenditure per pupil, value-added gain, and the performance index, with controls for enrollment changes

Outcomes	(1) 1 year change (2012-13)	(2) 2 year change (2012-2014)	(3) 2 year change (2012-2014)	(4) 2 year change (2012-2014)
Total Revenue	2.138***	1.190	0.557	-0.0261
	(0.822)	(1.071)	(1.006)	(1.062)
State Revenue	1.115**	1.128*	1.158*	1.186*
	(0.467)	(0.639)	(0.600)	(0.633)
Local Revenue	0.209	-0.555	-1.016	-1.441**
	(0.587)	(0.667)	(0.627)	(0.662)
Federal Revenue	0.813***	0.617***	0.415**	0.229
	(0.186)	(0.187)	(0.174)	(0.183)
Total Expenditure	0.447	0.356	-0.0167	-0.360
	(0.765)	(1.021)	(0.960)	(1.012)
Instructional Expenditure	0.329	-0.0488	-0.379	-0.683
	(0.477)	(0.584)	(0.549)	(0.579)
Gain	0.000986	-0.000249	0.000418	0.00103
	(0.00186)	(0.00171)	(0.00160)	(0.00169)
Performance Index	-0.00238*	-0.00428**	-0.00401**	-0.00375**
	(0.00144)	(0.00170)	(0.00159)	(0.00168)
N	608	608	608	608

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ and standard errors are in parentheses.

Each cell in the table represents its own regression. New poverty data from the 2010 Census was first used in 2012-13 (or FY 13). Calculated changes in Title I instrument for actual changes in Title I. All regressions control for district-level changes in state and local revenue per pupil from 2010-2012 as well as enrollment changes (2012-13 for the 1 year change, 2013-14 for the 2 year change). In column (1), the FY 13 IV is used for the actual change in Title I from FY 12 to FY 13 to assess the impact on the change in outcomes for FY 13. In column (2), the FY 13 IV is used for the actual change in Title I from FY 12 to FY 14 to assess the impact on the change in outcomes from FY 12 to FY 14. In column (4), the FY 14 IV is used for the actual change in Title I from FY 12 to FY 14 to assess the impact on the change in outcomes from FY 12 to FY 14. In column (3), both the FY 13 and FY 14 IVs are used for the actual change in Title I from FY 12 to FY 14 to assess the impact on the change in outcomes from FY 12 to FY 14.

Table 7

Descriptive statistics for enrollment, Title I allocations, and outcome variables, including revenues, expenditures, and student achievement for 2012

Variables	Mean (Standard Deviation)
Total Children	3242.320 (6006.637)
Average Daily Membership	2881.332 (4829.567)
Title I Revenue	212.990 (148.432)
Basic and Concentration Revenue	132.0358 (78.489)
Total Revenue	10282.180 (2010.946)
State Revenue	4770.588 (1423.497)
Local Revenue	4727.094 (2184.860)
Federal Revenue	784.499 (425.282)
Total Expenditure	9938.926 (1932.808)
Instructional Expenditure	5514.924 (1037.807)
Gain	.397 (1.294)
Performance Index	99.135 (6.056)
N	609

Revenue and expenditure variables are based on per pupil measurements. Total Children is the number of children, aged 5-17, who reside in the district, and this data was obtained along with Title I allocation data from NCES. Average Daily Membership is the total number of public school students residing within a district's boundaries, and this data was obtained from the CUPP Reports provided by the Ohio Department of Education.

Table 8

Descriptive statistics for exogenous and actual changes in Title I funding per pupil for 2013

Variables	Mean (Standard Deviation)	Maximum	Minimum
Exogenous Change in Title I	14.169 (33.242)	137.088	-334.323
Actual Change in Title I	19.274 (52.274)	541.630	-158.670
N	609	609	609

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